From the general trend of his first article (NATURE, October 6) I gathered that the reviewer was an advocate of the "solution" theory of Sir John Murray, and by carefully reading his second contribution (October 27) I have not entirely dispelled this impression. Yet he says, "I do not regard the lagoon in an atoll, which was formed, as Darwin suggested, by subsidence, as covering a reef at all."

This would seem to suggest a belief in Darwin's theory, and, if it is the case that the reviewer upholds this theory (as well as the opposed one of "solution") it may be well to point out that I too would not regard the lagoon of an atoll, formed by subsidence, as covering a reef. I should not have imagined it probable that anyone would so regard a lagoon were it formed in such a manner. The essential difference between such a view and the one that I have attempted to uphold is that I do not regard the lagoon as being formed by subsidence at all; but I do look on the lagoon as being a "slightly submerged reef" having a raised rim upon which islets are developed. Does the reviewer genuinely regard the lagoon as being formed by subsidence? If he does, why does he also plead the opposed theory of solution, and appeal to the *elevated* islands of Fiji? If he does not, why does he urge the statement as an argument against my views?

I am glad to see that he is prepared to admit that the various well-known phases of development of atoll-shaped reefs are "indirect evidence" of the truth of what I have maintained; but the Funafuti bore, he thinks, does not support it. The reviewer states that he does not think "the borings in the lagoon at Funafuti suggest a reef such as surrounds a lagoon." I should not have expected them to have suggested a reef such as *surrounds* a lagoon, for that reef is a consolidated and specialised "breccia platform." What might be expected is that such a bore would show the characters of a submerged reef—the open coral bank—*plus* the lagoon accumulations added since the completion of the atoll.

When such a successful bore is driven we may look for such appearances; but it is surely within the knowledge of the reviewer that the only bore at Funafuti which met with any success was *not situated in the lagoon.* The lagoon bore ("bore L") penetrated only 144 feet, and then failed; the only successful bore (on the results of which alone any safe argument may be based) was situated on the seaward reef, far removed from the lagoon. The successful bore ("main bore"), which reached a depth of 1114 feet, was driven on the extreme windward edge of a large atoll reef. In such a situation one would confidently expect the bore to penetrate the talus slope of the outwardly growing reef, and, from the description of the core obtained, it would appear that this expectation was realised. The Funafuti "main bore" tells little of the development of atolls save that they grow to windward on their own talus slopes—a fact hardly requiring a laborious boring for its acceptance.

The "L bore" can support no particular theory by reason of its very incompleteness; but such evidence as it does afford in no way contradicts, but rather goes to support, the supposition that it penetrated the lagoon debris of a submerged reef.

Whether the reviewer regards the Funafuti boring as evidence supporting Darwin's theory of subsidence or Sir John Murray's theory of solution I cannot quite determine; but he next defends the solution theory in the case of the Fijian Islands. He says that these islands have reefs "which superficially appear to be of the ordinary coral-reef type. Such reefs cannot have existed when the islands were first elevated, and it seems to me that Agassiz's photographs show that high islands do crumble to pieces within the calm of encircling barrier reefs." I own that I fail to follow this argument, for, granting that the reef is new since the island was elevated, what proof —or what probability—is there that the coast erosion was not present before the development of the reef, when the same condition is seen quite apart from reefs, or any other coral structures, all over the world?

The problem of the formation of coral structures (fringing reefs, barrier reefs, open reefs, atoll-shaped reefs, and atolls) is not, I think, to be solved by appeals to a multitude of opposed theories, and no critic's position is likely to gain strength by a series of fallacious arguments based alternately on the theory of subsidence, the theory of solution, and the results of the Funafuti bore.

F. WOOD-JONES.

St. Thomas's Hospital Medical School.

As a reviewer I would point out that I do not desire to uphold any theory, but merely to show what is good and what is bad in the book which I am reviewing, what facts are new, how far these and other facts support any theories, &c. An essay on the duties of a reviewer might be a suitable suggestion to the Editor of NATURE, but obviously I am not the author to present such an article.

In the first paragraph of Mr. Wood-Jones's letter of October 27, I am practically accused of being an "anonymous destructive critic" of, I suppose, the constructions erected by the facts brought together by Mr. Wood-Jones, some of them new and some old. I regard some of the bricks of his building as faulty, and I scarcely think there are enough bricks with which to complete the building. I intended to indicate in my review that I considered that science had gained by the attempt to build, and I desired indirectly to indicate some of the bricks which I thought future workers should attempt to collect. I do not believe any researcher on the coral-reef problem will consider my review as in any way unfair if he regards (*as I did*) Mr. Wood-Jones's book as a *contribution to science.*

I shall after this letter not continue this correspondence, not caring for Mr. Wood-Jones's style of writing. I would, however, make myself clear on two points. Mr. Wood-Jones admits that he assumes the lagoon of an atoll to be a slightly submerged reef. I point out that the nature of the material underlying the lagoons of atolls is doubtful. I appeal to the lagoon boring at Funafuti as giving the most valuable facts we have as to its nature. Do these facts, the best known geographical facts, support the theory of a *slightly submerged reef,* such as is supposed to exist at Cocos-Keeling? Down to 27 fathoms the first Funafuti lagoon boring passed through lagoon debris, and from that depth to 41 fathoms there occurred some firmly compacted masses of coral rock. In the second boring, which was carried to nearly 36 fathoms, a similar section was obtained. I do not consider that these two borings are sufficient to justify Mr. Wood-Jones's assumption, and I did not consider that the evidence given as to Cocos-Keeling lagoon justifies it. I quite fail to remember any description of the material under the Cocos-Keeling lagoon such as would suggest the open coral bank which is mentioned in Mr. Wood-Jones's letter, while its shallowness made it a peculiarly favourable place for investigation.

The fringing reefs round the high limestone islands in Fiji I certainly am inclined to regard as platforms left at low tide-level when those islands were washed away. In this sense they are new. They formed part of the bases of the islands when they were first elevated. Possibly the edges of these platforms have extended seaward since the land was removed by solution, and, still more important, by the erosion of the numerous small particles carried in the swirling waters. I consider these views are amply supported by published evidence. High limestone islands are also being washed away within barrier reefs, and I think it is a fair inference from the evidence that many of these barrier reefs were once similar shelves cut out from the land, or, to put it another way, left behind when the land was removed.

THE REVIEWER.

Note on Winter Whitening in Mammals.

I HAVE just seen a letter in NATURE of March 24 by Miss I. B. J. Sollas, in which, commenting on Mr. Mudge's observations, it is suggested that the yellow body produced artificially by Mr. Mudge in the fur of the albino rat is a substance similar to the yellow pigment of the stoat's winter coat, and therefore probably represents a stage in the reduction of the pigment to the condition in which it exists in the white hairs.

I had previously read Mr. Mudge's observations with great interest, and had suggested to him that they would throw light on the hitherto unexplained yellow tints in

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the fur of the winter-whitened stoat, as well as in the permanently white polar bear. I think Mr. Mudge's observations are a distinct help to us in getting at the meaning of these white coats. I should like to see what Miss Sollas can do with the hair of the variable hare, as in the whitened specimens of this animal I have never seen any trace of the yellow tints found in the stoat.

Mr. Mudge's note that the white areas of a piebald mouse can be turned pink by immersion in 5 per cent. nitric acid in 78 per cent. spirit, but only in summer or a warm temperature, is also of great interest. Does it not suggest a reason why pink colour in feathers is mostly found in summer plumages and in warm climates? And is not his production of brown in the hairs of white rats exposed to damp warm weather comparable with the well-known saturated tints so prevalent in animals living naturally in damp but warm countries?

While writing on winter whitening it may be well to direct attention to another point, which has always been difficult to explain on physiological grounds, namely, the fact that the black ear tips of the hare and the black tail tip of the stoat are not subject to winter whitening. This, however, would be explicable if, whereas the general body coat of both these animals is cast twice a year, the black hairs on the ears and tail are renewed only once a year. If they are renewed only once they must remain (apart from fading) of the same colour throughout the year. That such a single moult is possible, and even probable, in these two instances is shown by the fact that in the squirrel there are two moults of the general body coat, but only one of the ear tufts and tail hairs. Similarly in the Equidæ (according to Ewart), there are two moults of the general coat but one only of the mane and tail.

G. E. H. Barrett-Hamilton.

Kilmanock House, Campile, Co. Wexford,
Ireland, November 3.

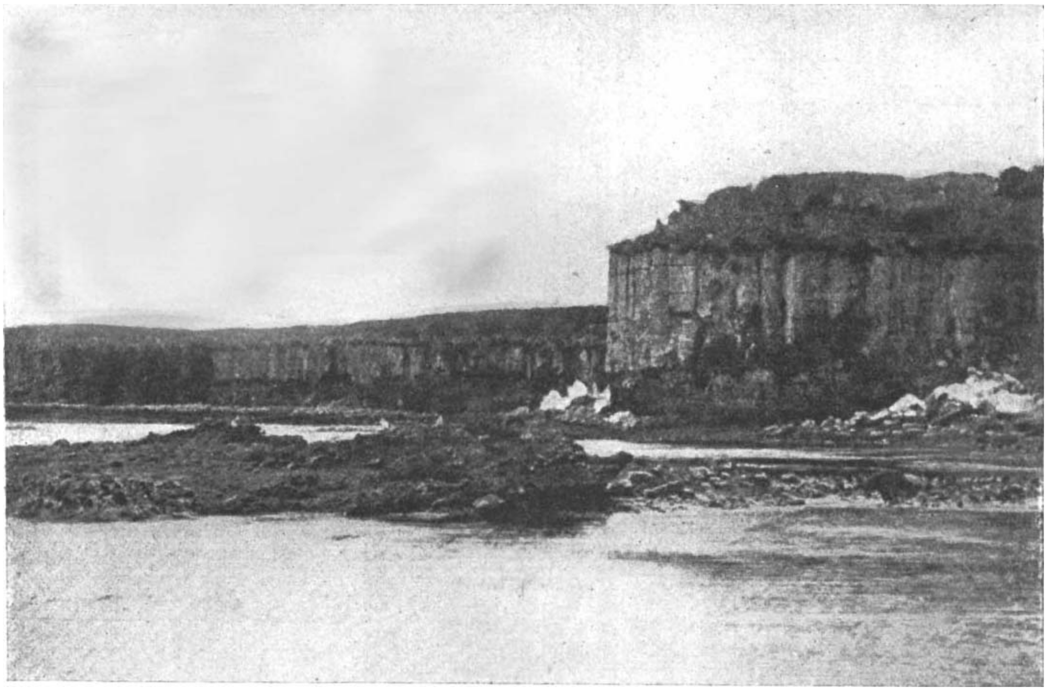

Fig. 1.—Cliffs of Columnar Basalt, Enderby Island. From "The Subantarctic Islands of New Zealand."

Helium and Geological Time.

I must apologise for an error in my letter published in Nature of November 3. The sixteenth line and onwards should read ". . . for we have no knowledge of chemical affinity between helium and solid substances; while, in respect of solubility, it would probably be inferior to the other gases."

R. J. Strutt.

Imperial College of Science, South Kensington.

THE SUBANTARCTIC ISLANDS OF NEW ZEALAND.[1]

THE naturalists of New Zealand have always shown themselves eager to take advantage of any opportunity for extending our knowledge of the fauna and flora of their country. Such opportunities are presented from time to time by the periodical official visits of the Government steamer to the outlying islands. In November, 1907, the s.s. *Hinemoa* deposited a large party of New Zealand men of science on Auckland and Campbell Islands, calling for them again on her return trip more than a week later. The expedition was undertaken at the instance of the Philosophical Institute of Canterbury, primarily for the purpose of extending the magnetic survey of New Zealand to the outlying southern islands, but the volumes before us consist chiefly of zoological and botanical observations, though there are also articles on geophysics and geology.

The work has been issued under the editorship of Dr. Charles Chilton, and the publication has been rendered possible by a substantial subsidy from the New Zealand Government. It comes at an opportune moment, and acquires a special interest in relation to the exploration of the Antarctic continent now in progress.

The time at the disposal of the expedition was, of course, all too short for a complete biological survey, and the collections were evidently, at any rate in many cases, very fragmentary, but many very interesting results were obtained. The zoologists were undoubtedly right in devoting most of their energies to the terrestrial fauna, which is much more likely to be modified or even exterminated by human agency than the marine fauna, but we cannot help wishing

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[1] The Subantarctic Islands of New Zealand. Reports on the Geo-Physics, Geology, Zoology, and Botany of the Islands lying to the South of New Zealand. Based mainly on Observations and Collections made during an Expedition in the Government Steamer *Hinemoa* (Capt. J. Bellons) in November, 1907. Edited by Prof. Charles Chilton. Vol. i., pp. xxxv+388, vol. ii., pp. 389–848. (Wellington, N.Z.: Philosophical Institute of Canterbury. London: Dulau and Co., Ltd., 1909.) 2 vols. Price 42*s.* net.

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